LAKATOS ON PROOF AND ON MATHEMATICS

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Hugh Lehman has masterfully presented 'An Examination of Imre Lakatos Philosophy of Mathematics' (Philosophical Forum, 12, 1980, 33-48): since Lakatos was a skeptic, he asks, why should he have bothered with proofs at all? Lehman answers this question for Lakatos: skepticism was traditionally deemed to be the despair of reason, and Lakatos wanted to present a rationalistic skeptic view of mathematics – which made him study the role of proofs as something other than dispelling all doubt. Moreover, according to Lakatos, Lehman notices, the boot is on the other foot: the anti-skeptics cannot offer a good theory of the role of proof in mathematical reasoning, since dispelling doubt is not mathematical progress.

Lehman is thus led to the view Lakatos offers of the nature of proof – his theory of proof-analysis and of counter-example – and to its examination. He concludes that Lakatos partly describes the tradition in mathematics, but partly proposes a reform of that tradition. Let me repeat here my observation that this is not unique to either Lakatos or to the theory of mathematics: a new theory of science, of art, of religion, of economics, etc., will of necessity come up against the same situation: new explanation implies reform in all spheres of human action as long as the action is an ongoing concern.

Lehman examines critically the view implicit in Lakatos concerning acceptance or rejection of this or of that mathematical theorem. For my part, I agree with Lakatos that acceptance of objects of belief is never an issue: what an investigator believes or disbelieves is his private affair. Lakatos endorsed Sylvain Bromberger's distinction between acceptance of an object of belief and acceptance of an object for investigation. For my part, I think acceptance can be for a plethora of purposes. But I need not elaborate this point here. Lehman's reference to Peggy Marchi – Lakatos' former student and leading exponent – and her presentation of theorems as explicanda suffice here, since Lehman shows that proof-analysis Lakatos-style is a form of explanation in a number of senses. So much for a brief abstract.

Lehman's contribution is important, and in a way rather overlooked by him. It can be stated in two brief sentences. *Lakatos' theory of proofs comes to solve a problem for the rationalistic skeptic. It is thus not meant to be a comprehensive theory of mathematics.*

When I first met Lakatos, he told me of his ambition to view mathematics as a whole in the light of developments from Frege and Russell through Skolem and Löwenheim to Gödel and Church. Only fragments of his early view are extant. He had divided already then the whole field of mathematics into the pre-formal, and the formal, and the post-formal. He noticed that definitions are often used as a means for escaping valid criticism, and he wondered when such escapes are regressive (monster-barring), when progressive (the axiomatization or the formalization of significant, crystallized systems). He fully agreed that fully axiomatized – purely formal – systems are immune to criticism, and was willing to relinquish all skepticism regarding them. But he declared them outside mathematics proper in the sense that they deserve the research efforts of robots, not of humans. Paradoxical though this thesis sounds, if not even perverse, no mathematician will disagree with it, though the above presentation of it is unorthodox and even, to speak empirically, quite annoying for some anti-skeptical mathematicians. Yet, to be clear, though purely formal studies are for the robots, the formal systems are themselves of great interest for mathematicians proper; especially, said Lakatos, since their interpretations are conjectures.

All this was clear, to repeat, in Lakatos' earliest stage, and in his first exposition of his views I heard he made it clear that he was aware of the significance and explosiveness of his ideas. Yet he was driven to study the problem Lehman so masterfully presents and never returned to his overall view. He died young. In his last years, he moved from the philosophy of mathematics to other activities. And, to conclude, he was then fully aware of the deficiency of the views he had presented in his masterpiece, *Proofs and Refutations, which he intended to rewrite and correct*. The defect was already studied by Peggy Marchi in her doctoral dissertation written while she was a student in the London School of Economics with Lakatos as her supervisor: he took as his initial framework Popper's classic *Logik der Forschung*, considering a theorem as a given and the task of the mathematician that of providing a proof, i.e. an explanation (to put it in Marchi's way). Yet the question (a) why pay attention to one fact or theorem and not to another is not sufficiently examined by either, nor the question (b) how do conjectures evolve.

As to question(a), the facts that call our attention are often the count examples to earlier conjectures. But this will not suffice: some ventures go on – a series of conjectures and refutations – then are aborted, then taken up again with renewed interest. There are trends or fashions in the history of ideas, and this fact calls for more explanations of intellectual trends by reference to changing metaphysical frameworks. As to question (b), certainly Popper and Lakatos were right to ascribe an important role to intuition as opposed to the view of science as developed without intuition, algoristically, by a kind of sausage-making machine. They both referred to Henri Bergson with approval (in spite of his not being a skeptic at all, and hardly a rationalist). Nevertheless, there is more to the matter at hand since, as I think Russell Norwood Hanson was the first to suggest in this context, between algorism for research and no guideline at all there are partial algorisms. These, I have ventured to show, are partially generated by metaphysical systems.

To conclude, Lakatos did not demarcate mathematics, did not explain axiomatization or formalization, did not demarcate premature axiomatization or formalization from proper axiomatization, did not discuss the impact of general mathematical or even extra-mathematical ideas on research projects. He 'only' presented a rationel skeptical theory of mathematical proof. This makes his work seminal and highly future-oriented. He thus generated a new research program in the philosophy of mathematics which (being a rationalistic sort of skeptic myself) I hope will soon become the paradigm.

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